



DEPARTMENT OF VETERANS AFFAIRS  
Medical Center  
2002 Holcombe Boulevard  
Houston TX 77030

November 4, 1991

In Reply Refer To:

\* Dr. John B. Scythes  
32 Beaty Avenue  
Toronto, Ontario M6K3B4  
Canada

Dear Dr. Scythes:

Thank you very much for your interesting letter of October 20, 1991. I am not sure which paper of mine Rolfs thought was unimportant, but the CDC thinks that about all of them, so it doesn't matter very much.

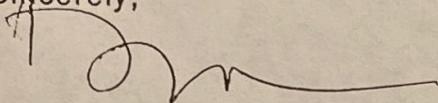
I looked at the abstract for a workshop discussion on Friday, December 6, 1991 at the Second Annual Conference on HIV/AIDS. Where the issue of syphilis is raised, there are, in my opinion, too many highly conjectural statements to render a successful argument. I have no argument with the data on incident of untreated cases of syphilis or the new infection rate, and I agree that it is not clear what course syphilis would follow in immune-suppressed individuals. However, I'm not certain what the author of this abstract means when he states that, "no studies support acceptable sensitivity of these tests for re-infection." Reinfection did not occur in the pre-penicillin era, and has been a common and regular feature of sexually transmitted diseases in the penicillin era, during which time the VDRL has been thought to have the same level of reactivity as in a primary case. I would be interested in seeing literature that provides data disputing this statement. Thus, I am not aware of studies which show only a 50% reactivity rate in reinfection. Furthermore, I do not really agree with the statement that reactivity in the treponemal test is strongly associated with VDRL positivity. I just do not understand the statement that immunity to syphilis is easily confused with tolerance, even though the authors state that many animal and human studies have shown this. As for the animal model that suggest that *T. pallidum* can induce AIDS, I am aware of no study that is even modestly convincing. In the closing paragraph, the author states that cell mediated immunity may need to be boosted by purified antigen inoculation; to my knowledge, there is no system that is known to do this.

\* I have advised Dan Musher re this error.

Dr. John B. Scythes  
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To my great disappointment, the article written by Mark Dowell and myself on the effect of ceftriaxone in treating latent and asymptomatic neurosyphilis in HIV-infected subjects was turned down by the *Annals of Internal Medicine* with negative, and rather hostile, reviews. I would not be surprised if one or more of those reviewers was from the CDC. At any rate, I shall rewrite the article and submit it elsewhere. Thank you for sending me the interesting information. With best regards, I remain,

Sincerely,



Daniel M. Musher, M.D.  
Chief, Infectious Disease Section  
Professor of Medicine,  
Microbiology/Immunology

DMM/ic